

Wind Energ. Sci. Discuss., referee comment RC1
<https://doi.org/10.5194/wes-2021-21-RC1>, 2021
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Comment on wes-2021-21

Anonymous Referee #1

Referee comment on "An analytical solution for wind deficit decay behind a wind energy converter using momentum conservation validated by UAS data" by Moritz Mauz et al., Wind Energ. Sci. Discuss., <https://doi.org/10.5194/wes-2021-21-RC1>, 2021

The paper sets out to develop an analytical far-wake model and compares model results to UAS obtained wake measurements. The topic is interesting and so is the measurement technique employed, unfortunately though, the paper should be rejected for a number of reasons.

Firstly, there are issues with the most important part of the paper, namely the wake model description and development:

- Inconsistent and incomplete description of the analytical wake model
- Underlying model assumptions and their significance are not explained
- The reasoning for certain model choices remain unanswered and cannot be followed
- None of the model subcomponents are verified against existing datasets of which there exist many
- The model is only valid below rated wind speed

Unfortunately, the described wake model is not thoroughly derived nor is its derivation sufficiently innovative with respect to existing models to justify the flaws in the model development and description to be overlooked or fixed in a major revision. An underlying issue is that the need for such a model remains unclear, as it is not an improvement over the status quo. Important recent developments are not mentioned in the introduction and have been missed.

Secondly the comparison with measurement data is flawed.

- The measurement campaign is not sufficiently described, the only information consists of the three flight paths taken by the UAS and its sensory equipment. The measurement period and number of data points at each position in the wake is not given. Were there really just three flights?
- As there were only 3 flights, the dataset is not statistically significant. The authors do not comment on this issue and so it also seems bizarre to compare instantaneous data

(slightly time-space filtered as they employ a time filter but the plane is in-stationary) to analytical models that are derived under steady-state assumptions, including their own. The authors miss that the wake spreading coefficients of the other analytical models (including the one they use for their model) are only valid for time-averaged solutions. One can use these models anyhow and retune the wake spreading coefficient for instantaneous wake computations as well, however it did not seem as this was the ambition of the authors nor do they seem to be aware of this issue.

The authors are advised to restructure and rewrite the model development and use some of the existing, already published wake measurement datasets and existing models available to verify and validate their model. However, this would entail writing a paper from scratch. This paper should be rejected.

Attached are some more detailed comments to the authors on some essential issues.

Please also note the supplement to this comment:

<https://wes.copernicus.org/preprints/wes-2021-21/wes-2021-21-RC1-supplement.pdf>